Author’s response to reviews

Title: When Female Circumcision Comes to the West: Attitudes Toward the Practice among Somali Immigrants in Oslo

Authors:

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Author’s response to reviews: see over
Major Compulsory Revisions:

Q. The study is based on a respondent-driven sampling method. The research design is well described but we don’t know anything about the number of observations in the final sample (by sex, age, education and migration history).

We appreciate reviewer’s suggestions and we added a descriptive table in the manuscript accordingly: See table 1.

Q. and the wording of the questions, especially regarding the dependant variable.

The dependent variable was explained fully: See INDICATORS section, page 6, line 4-9.

Q. Firstly, it lacks descriptive information on the composition of the sample. For example how many of respondents are married? How many of them have daughters?

See table one

Q. Secondly, the table 2 is too complicated and would be more appropriate with a separate analysis by sex.

Table 2, which is table ‘4’ now, is based on logistic regression model. There was some computer errors that made the table difficult to read, and we fixed them now to make the table more readable and comprehensible. Table 4 is important to be in the manuscript to help readers understand about factors that are associated with the continuation of FC in study population, when effect of gender is controlled. But we elaborated gender differences in table 2.

Q. It seems difficult to avoid an analysis of gender differences in attitudes toward female genital cutting, but the authors fail to do this in the statistical analysis. A revision of the article should definitely address this gender dimension.

Gender differences as regards to socio-demographic variables as well as knowledge and attitude variables were addressed. See table 2. See also, page 5, the result section, first paragraph. You can also see in the discussion section, the last paragraph of page 10.

Q. In the quantitative analysis, more descriptive information in a cross table between dependant variable and the socio-demographic details should be provided.

These details are available in table 3

Q. The main argument of the article is that Female Genital Mutilation may lose his status-related dimension due to the “social transformation” (i.e. assimilation? Where does this social transformation is supposed to occur and for whom?) which ‘alter immigrants’ attitudes and behaviors toward this
longstanding tradition». The premise of the analysis does not take sufficient account of the specificity of the migration experience and the possibility for people for maintaining ties with the society of origin. Actually, some qualitative researches show that in a migration context, female genital cutting is still positively perceived by migrant, especially in the context of endogamic marriages or prospects of return to the country of origin.

We recognize the importance of this issue and we acknowledge that the migration factors that may led to the observed change require further investigations and explorations. Nevertheless, we have discussed the main issues that may be altering Somalis immigrants attitude towards the practice (See the discussion section, page 10, paragraph 2), more detailed information and analysis of those issues can also be found in our earlier qualitative article which is available at (http://www.dovepress.com/articles.php?article_id=9107).

The pressure from home country, particularly grand mothers who want girls to be circumcised, is a very important point, as it may have an effect on immigrants’ attitude towards the practice. According to our earlier qualitative findings, such pressures exist among Somali immigrants in Oslo. Based on respondents’ perspectives, they resisted to grandmothers pressures regarding circumcising their daughters. It is worth mentioning that the Somali community in this study have lived in Norway on average of 9 years, vast majority of them are still new to the country. Their migration began following the civil war in Somalia in 1991 which continues to-date. However, according to Norwegian Statistics Bureau, the vast majority of Somalis in Norway came after the year 2002. Most of girls who born or brought up in Norway are still too young to marry. Moreover, going back from Norway to Somalia is presently very rare as the security situation in the country is bad. Thus the effect of endogamic marriage cannot be predicted at this stage, we may wait and see when more girls (uncut), who born in Norway, grew up and begin to choose their partners back home.

Q. much more likely to attract “boyfriends” but what about “husbands”? In the discussion section authors may add a comment on this point.

This statement was referred to a previous qualitative article. And according to that article girls attract boyfriends and they are also more likely to be married which means they are more likely to attract husbands too. Quoting our earlier article “the vast majority of male participants indicated that they want to marry uncut girls. Since most Somali men in Oslo prefer to marry uncircumcised over circumcised women, it is unlikely that parents in Oslo will circumcise their daughters knowing that FC could only make their daughters worse off”. See this article for detailed information about the issue “Attitudes toward female circumcision among Somali immigrants in Oslo: a qualitative study”

Q. The discussion focuses on the fact that Somali men who live in Norway are more likely to have positive attitudes toward female genital cutting. The statistical analysis shows that 40% of the male respondents favor mutilated women to be their wives. It would be interesting to highlight the correlation between these attitudes and the marital status of those male respondents (and the situation of their wife regarding female mutilation).
We have tried to make this analysis but we have seen ‘no differences’ among marriage groups regarding their preferences in circumcised vs uncircumcised wife. We fully agree that it would be interesting to see the correlation between men’s preferences and the FC status of their wives, but unfortunately, we don’t have such data.

Minor Revisions:

Q. The article use the term of Female Circumcision which is close to the vernacular term. But, in a public health journal, it seems more appropriate to use the WHO terminology of Female Genital Mutilation/Cutting.

While we recognize the gravity and severity of the practice, the term female circumcision (FC) has been used in our study publications, because the term ‘FC’ is the term which is most acceptable to the community under study. Moreover, according to Somali language, the term FGM does not cover all types of the practice but it is a direct translation of type 3 (pharaonic), while the term FC covers all forms. More information about this can be found in the recent master thesis (2012) by Ingvild Bergum Lunde which will be published soon in Oslo University website http://www.duo.uio.no/

Q. The authors introduce their research saying that it is the first quantitative study in Europe investigating the abandonment of female genital cutting in migrant African family. But it seems that a similar survey take place in Spain and was published in 2010 in the BMC Public Health: see the article of A Kaplan-Marcusan, N Fernandez del Rio, J Moreno-Navarro, MJ Catany-Fabregas, M Ruiz Nogueras, L Munoz-Ortiz, E Mongui-Avila, P Toran-Montserrat. Female Genital Mutilation: perceptions of healthcare professionals and the perspective of the migrant families, BMC Public Health, 2010, 10:193

*Thanks for this important point, we intend to say the first quantitative study “using probability sampling methods” and investigating the abandonment of female genital cutting in migrants in the west. See page 4, paragraph 3, last 3 lines.*

Q. Female respondents were asked if they have been mutilated as well as the type of their mutilation, but how many of them simply ignore the type of their mutilation?

We have recently collected validation data about the reliability of self-reported type of FGM and the reality obtained through clinical examination among Somalis. We didn’t analyze this data yet. Thus, it is too early to estimate the level of uncertainty associated with self-reported type of FGM in this study.

Q. In table 1, p-values are missing

*In table one, now table three, we reported confidence interval (CI) instead of  P – value, which we beleive is preferable.*
Reviewer 2

1. I wonder how the sample size was calculated when the authors on page 6 first paragraph write that “The chain referral process continued until the desired a priori sample size of 214 was obtained”. It would be nice if they write a sentence about it.

A sentence clarifying the calculation formula was added according to the reviewers comment. See page 6, paragraph one, the last three lines. (Calculation was performed using beta = 0.20 and alpha = 0.05 with sampling error of 2. We assumed 60% of attitude change with reference to original population, where 100% of the people are positive towards the practice. This gave us a sample of 107*2=214).

2. When the authors present descriptive results in percentages, they should make sure that the sum will be 100%. I found that the sum of the different types of education presented under the result section in paragraph 1 makes it 100.8% (7.8+53+31+9).

Thanks to the reviewer, we corrected the calculation of proportions.

3. On page 10, paragraph 2 the authors have reported Odds ratio which I think is not that usual to have such figures in the discussion section.

Thanks to the reviewer. The odds-ratio is now deleted from the discussion.